2.9 BLOWOFF AND ESCAPE OF H_2

D. M. Hunten

Introduction

This discussion is a summary of a recent paper (Hunten 1973a, herein referred to as Paper I) on the escape of hydrogen from Titan. Some extensions and related material will appear in two other papers. Paper II (Hunten 1973b) improves one of the derivations and applies the ideas to several different atmospheres. Paper III (Hunten and Strobel 1974) is a detailed analysis for the Earth, which verifies the general principles discussed here and in Paper I.

Trafton's (1972) announcement of the possible presence of H_2 on Titan included a discussion of its loss by Jeans escape. He did not find a way to specify the height of the critical level, from which this molecular loss can be regarded as occurring. In addition, he implicitly rejected the possibility of hydrodynamic loss, or "blowoff", which can give much greater loss rates under certain conditions. The treatment of Paper I supplies both these missing ingredients. First, it is shown that a pure hydrogen atmosphere cannot be retained by Titan, but will blow off in a few hours. Addition of a heavier gas, such as CH_4 or N_2, in comparable abundance gives a great improvement, although the escape rate can still be large. Moreover, the actual flux can be predicted with confidence from the mixing ratio of H_2 to heavy gas.

H_2 Blowoff

The instability of an H_2 atmosphere follows from its large scale height $H_1 = kT/m_1 g$, which is not much smaller than the radius r_o of Titan. A convenient variable is λ, which is essentially the ratio of gravitational and thermal energies:

$$\lambda = \frac{GMm_1}{kTr} = \frac{r}{H_1} \qquad (1)$$

The symbols are: k, Boltzmann's constant; T, temperature; m_1, the mass of an H_2 molecule; g, the acceleration of gravity; G, the gravitational constant; M, the mass of Titan; r, the distance from its center. My illustrations were for a somewhat arbitrary constant temperature, T = 100°K, but are not grossly changed for other likely temperatures. For this temperature, $\lambda_o = 8.6$ at the surface, and it varies as 1/r. Indeed, the hydrostatic equation for the number density n can be written

$$n = n_o e^{\lambda - \lambda_o} \qquad (2)$$

Even at an infinite distance $\lambda_o - \lambda$ is only 8.6, and the density is only reduced by the factor exp(8.6) = 5400. The static situation is unstable, and will be replaced by a planetary wind, whose description resembles that of the solar

wind. In that phenomenon, the speed is found to be sonic at $\lambda = 2$, a distance of 4.3 r_o or 11,000 km from the center of Titan. The time constant for this blowoff is found to be only 4 hours.

This description is unlikely to be modified by changes in the temperature profile, unless the expansion is so rapid that adiabatic cooling overcomes conduction and solar heating to produce boundary temperature of 10-20°K...

Danielson: It will fall back in then.

Hunten: Yes, perhaps as clouds of condensed hydrogen (Sagan 1973). I don't think the situation is stable, because without very rapid outflow the adiabatic cooling cannot overcome the heat sources. A preprint by Gross (1973) begins to attack this problem by modeling the atmosphere as a polytrope. In a sense, my adoption of a constant temperature implies an exact balance of adiabatic cooling with solar heating.

Pollack: The normal balance in a planetary atmosphere is between heating by the Sun and radiation in the infrared; I really don't understand this adiabatic atmosphere.

Hunten: That's normally true; but I suspect that adiabatic cooling would become important as well in this blowoff situation. I don't really believe in the pure hydrogen atmosphere anyway, because it would require a huge source.

Strobel: Adiabatic cooling can be important if the time scale of the expansion is short compared to the time constant for heating.

Hunten: That's the idea; the time scale for this flow is less than an Earth day. If the same reasoning is applied to heavier and heavier gases, with their larger values of λ, stability against blowoff is found for masses greater than 6, although evaporation by the Jeans process can still take place. Thus, methane, with its mass of 16, is safe unless the temperature is considerably greater than 100°K. This temperature is a kind of mean for the whole atmosphere, to be used in the barometric equation.

Trafton: The barometric equation doesn't apply in such a non-equilibrium situation.

Hunten: That's true; properly speaking, the acceleration of the gas must be included, and we get the equation of motion used for the solar wind. But the variation of density with height isn't much different, especially for the heavier gases that are stable, or almost stable, against blowoff.

H_2 Escape Flux from a Mixed Atmosphere

Now, to turn to the case of a mixture of H_2 with a heavier gas: if the composition doesn't vary with height, the mean mass can be used to test for blowoff. Thus, a 50:50 mixture (by number) of H_2 and CH_4, with a mean mass of 9, should be stable. But what about the tendency of the H_2 to rise through the CH_4, take up its own scale height, and approach the state of diffusive equilibrium? This is the really new idea in Paper I, where I introduce the idea of "limiting flux". (Actually, the idea has been used for terrestrial hydrogen for years; the new thing is just the application to other situations.) The H_2 escapes freely from the top of the atmosphere; thus, it is like a gas flowing through a barrier into a vacuum. We must assume an equal source in the lower atmosphere or in the interior of Titan. The resistance of the methane (or other gas) to flow is inversely proportional to the diffusion coefficient $D_1 = b_1 (n_1 + n_a)$. Here b_1 is a constant, the "binary collision coefficient", and n_1 and n_a are the number of densities of H_2 and the background atmosphere. The resistance therefore is less at greater altitudes, and the same H_2 flux is carried by a smaller density at a higher speed. In the limit of easy escape from the top, the result is a "limiting flux" ϕ_ℓ and a constant mixing ratio $f_1 = n_1/n_a$. (Note that f_1 is the ratio of H_2 to everything else, not to the total.) Specifically,

$$\phi_\ell = \frac{b_1}{H_a} \cdot \frac{f_1}{1+f_1} \left(1 - \frac{m_1}{m_a}\right) \tag{3}$$

Unless f_1 approaches or exceeds unity, this flux is proportional to f_1, and therefore really represents a "limiting velocity", inversely proportional to n_a as just discussed.

In Paper III, Strobel and I relate this simple, but general, description to a detailed model for the Earth's atmosphere, and find that the description is very satisfactory. If there is a temperature gradient, Equation 3 includes another small term involving thermal diffusion (Paper II).

If the actual flux is less than ϕ_ℓ, the H_2 density does not fall off as rapidly with increasing height, and much larger densities are obtained at great altitudes. This is the way the system tends to react if escape from the top is less than easy; the densities are larger and tend to maintain almost the same flux. The Jeans equation tells us the density required for a given flux, not the flux that is obtained for a given density. If escape is still more difficult, the system reverts to the usual description, in which the flux is derived from the density. But this situation is highly improbable for H_2 (and He) on Titan.

If the limiting flux is strictly realized, so that the mixing ratio is independent of height, then it does not matter whether or not the atmosphere is vertically mixed by eddy diffusion or other forms of vertical motion. These processes tend to produce a constant mixing ratio too, but they make no difference if that is already the case. Usually we lump all the mixing processes into an eddy coefficient, whose value is always a problem when we study an upper atmosphere. For our purpose, it is irrelevant.

Danielson: Surely that's only true up to some limiting mixing rate?

Hunten: Yes - if the mixing is too intense, there may be no part of the atmosphere where Equation 3 can be applied to get the actual flux. This is true for helium, and heavier gases, on Earth. I don't want to go into details now, but there is a discussion in Paper II, with a criterion to tell which situation applies to a given gas.

Pollack: In the lower part of the atmosphere, where mixing dominates over diffusion, how do you get a value for the flux?

Hunten: You're perfectly right -- in such a region the flux can be almost anything, but the H_2 mixing ratio is constant. To find the flux, we must find a region dominated by molecular diffusion, namely the region above the homopause (or turbopause). Some people call it the heterosphere, others the diffusosphere. In this region Equation 3 can be applied to get the flux. Paper I gives values of the coefficient that multiplies $f_1/(f_1 + 1)$; it is a little under 2×10^{12} cm^{-2} sec^{-1} for a CH_4 atmosphere, and twice as great for N_2. The temperature dependence is very slight, because b_1 varies as $T^{0.8}$ and H_a as T. If we focus on the methane atmosphere, we obtain the following typical limiting fluxes, in molecules cm^{-2} sec^{-1}:

f_1	0.1	1.0	2.5	10
ϕ_ℓ	1.7×10^{11}	9×10^{11}	1.3×10^{12}	1.7×10^{12}

The third case uses the 5 km-A of H_2 from Trafton (1972a) and the 2 km-A of CH_4 from Trafton (1972b). The second is the composition favored in Pollack's (1973) greenhouse model. The last entry, with a mean mass of 3.3, would have to be examined critically, because it is subject to blowoff. Even though the H_2 is blowing away, it does not necessarily carry along the CH_4, because they can part company at very high altitudes (Paper II).

Paper I contains a model of the outer atmosphere, or corona, for the modest flux of 1.3×10^{11} cm^{-2} sec^{-1}. The methane is confined to low altitudes, and above that the H_2 takes up its own scale height. At 4.3 Titan radii, where $\lambda = 2$, is the critical level, and the effusion velocity is 100 m/sec. Thus, the loss is still slightly short of being hydrodynamic. If the corona is not isothermal, as we discussed earlier, the critical level will adjust itself, but I don't expect any major difference.

Possible Sources of H_2

I am convinced that the fluxes shown in the table above are correct for the mixing ratios assumed. If we believe Trafton's data or Pollack's model, we must also believe that Titan has a source of H_2 amounting to 10^{12} molecules cm^{-2} sec^{-1}. If we do not like that, we must add something like the 50 km-A of N_2 that I suggested last year (Hunten 1972). The flux is then 1.7×10^{11} cm^{-2} sec^{-1}, which is still large.

Danielson: You can live with Trafton's abundances?

Hunten: Yes, if the flux is acceptable.

Trafton: But those abundances are very uncertain. There could be less methane if some other gas is present, and the laboratory calibration is another source of error. For H_2, the error could easily be a factor of 2 because it is hard to determine the continuum position.

Hunten: Anyway, your data and Pollack's greenhouse model seem to tell us to look for a strong source of H_2. Methane photolysis begins at 1550 Å, and the Sun provides only 10^{10} photons cm^{-2} sec^{-1} at shorter wavelengths, too small by a factor of 100...

Sagan: What about indirect photolysis, sensitized by some other molecule?

Hunten: Perhaps, but we can also think about NH_3. Paper I contains some estimates, based on a suggestion by Lewis (1971). There should be a small amount of ammonia near the surface, and it could just provide the source if everything worked at top efficiency. The solar radiation below 2250 Å would have to reach the surface, and the quantum yield for H_2 production would have to be high. Neither seems really probable to me, and Lewis has been telling me the same thing.

I am much more disposed to favor a source in the interior of Titan. Lewis' models suggest the presence of a strong NH_3-H_2O solution, which is fairly corrosive and might react with other things to make H_2. Again, it seems improbable but perhaps we should not rule it out just yet. Radiolysis by α, β, and γ emissions from radioactive decay is also a possibility. While a flux of 10^{12} cm^{-2} sec^{-1} seems large for an atmosphere, it is probably trivial for an interior.

I notice Lewis' eyebrows getting higher and higher, and he may want to say something. Nevertheless, I think we are logically forced to believe in the presence of some potent H_2 source, if we believe that large amounts of it are present in the atmosphere.

Possible Recycling from the Toroid around Saturn

What about the influence of the toroid of gas orbiting around Saturn, suggested by McDonough and Brice (1973) and the preprint by Sullivan (1973) that just reached us? Both papers point out that hydrogen can be recycled back to Titan's atmosphere, and suggest that the net loss rate may be much less drastic than suggested here and by others. I have not considered the question adequately, but I suspect that the response of Titan to such recycling will be to increase its outward flux to compensate, until the difference between the outward and recycled fluxes is again equal to ϕ_ℓ. The mechanism is to build up the coronal density by whatever factor is needed

to raise the outward flux. If the net flux is much less than ϕ_ℓ, the scale height of H_2 above the homopause will be large, instead of being equal to that of CH_4. Efficient recycling would probably require the density of the toroid to approach the density at infinity required by (2), with the reference density n_o taken at the homopause. Toroid densities of 10^{10} cm^{-3} or more might be required, far greater than any thing suggested by McDonough and Brice. Sullivan, on the other hand, adopts an approach similar to the one just outlined, and obtains a density as high as 10^{11} cm^{-3}.

McDonough: What size atmosphere are you talking about?

Hunten: In my example, the diameter is 8.6 Titan radii, big enough to sweep up quite a lot of hydrogen in your model. But the problem I see is that your toroid may need a very high density to be effective in recycling. Your model does not include the collisions between molecules, and they will dominate the situation if the density is as high as I suspect. Perhaps the collisions can be included, but the final model may be a very different one.

Sagan: A remark on the source of the hydrogen. Experiments in prebiological organic chemistry start with methane, ammonia, and water, but no hydrogen. As the irradiation proceeds, and organic molecules build up, and a substantial fraction of the initial mass of hydrogen becomes free H_2. If we believe that organics are made this way on Titan, we must also believe that H_2 is being produced.

Hunten: Certainly. But there is still a question about the total rate of production. At unit quantum efficiency, the process must use all the solar photons up to 2300 Å to get the inferred fluxes of H_2.

Sagan: But you can also do it by inefficient photolysis up to 3400 Å, as in our experiments, and that's not out of the question. The other possibility is to let the solar radiation drive the weather and produce thunderstorms. Visible light can do this and let you draw on a much larger energy source, converted inefficiently into electrical discharges.

Now, what about the material left behind by the escaping hydrogen? If 10^{11} to 10^{12} molecules cm^{-2} sec^{-1} escape from Titan over geological time, how much ice is volatilized? By a quick calculation, the column seems to be several kilometers deep...

Hunten: Or a comparable layer of organic compounds (see Paper I).

Sagan: Now, the idea of melting a few kilometers or a few tens of kilometers of planetary surface over geological time is not unknown to us on the planet Earth. I don't find anything bizarre about a similar turnover on Titan. What about methane and ammonia volcanoes, with liquid ammonia lava, to help with outgassing?

Lewis, Sagan: (Discussion of such volcanoes and the possibility of observing them through breaks in the clouds. Lewis feels that volcanic activity is at best no easier to generate under Titanian conditions than terrestrial ones.)

Hunten: At any rate, volcanoes will merely release gas into the atmosphere. They don't help make hydrogen out of it.

Lewis: I just did a quick calculation about radiolysis in the interior. If all the energy from radioactive decay in Titan were deposited in the right kind of material, and with the usual rule of thumb that 1 percent of it goes into new chemical bonds, there is no problem in making enough hydrogen for you. In fact, there is a factor of 1000 to spare.

Hunten: Provided the radioactive material is not segregated from the H_2O, NH_3, and CH_4.

Lewis: There should be plenty of water of hydration right in the silicates, provided the internal temperature did not get high enough to dehydrate silicates and physically separate all the water.

(Post-meeting Note: Regrettably, a later check on this estimate showed it to be far too optimistic. Lewis takes a chondritic radioactive energy release of 5×10^{-8} erg gm^{-1} sec^{-1} diluted by 3 times the mass of ices. If 25% of the energy is in 1 MeV β particles, and if they have a 1% radiolysis efficiency, the production rate per cm^2 of surface is 5×10^8 H_2 molecules. While this estimate might be raised somewhat by a detailed calculation, it is far short of the desired 10^{11} - 10^{12} cm^{-2} sec. The N_2 source mentioned below by Lewis is still of interest: the flux estimated above, divided by 3, would give an accumulation of 10 km-atm.)

Trafton: What about other icy satellites; shouldn't hydrogen be produced in them by the same process?

Hunten, others: Some satellites may not have internal convection to bring the gas near the surface. In any case, unless there is an abundant heavy gas, like methane, to retard the loss of H_2, a measurable density of hydrogen cannot be built up.

Lewis: According to experiments on, for example, organic crystals, radiolysis by 1 MeV β particles is almost 100 percent efficient in initially breaking chemical bonds. Many of them then recombine, but new compounds are also formed.

One last word on radiolysis. If it is taking place, I expect water to be the compound that is broken up to make H_2. The oxygen will react with ammonia rather efficiently to make nitrogen.

Danielson: Then why don't we get the same thing with the Galilean satellites? We should see nitrogen there.

Pollack: Yes, why don't some of the Jupiter satellites have nitrogen in their atmospheres? For two of them, there are occultation experiments that have set very severe limits on the surface pressure.

Lewis: There are several possible answers; a satellite may not contain ammonia, or the radioactivity may be segregated from the water and ammonia. Several conditions must be satisfied for radiolysis to be effective. I am only giving a plausibility argument for a source of H_2 and N_2.

Blamont: But several satellites may contain a hydrogen source?

Lewis: Yes, if only by radiolysis of water.